March 11, 1953

Dear Phil:

Undervsapparate cover, cultures named herein, and some hasty notes herewith. Most recent receipts: yours of the 6th, reprints, and bredenyy cultures, for which thanks.

		Comment
SW-977	zega —x Hines VAH	j?
SW980	abony b:enx —x javiana $1z_{28}$:1,5 $1z_{28}$:enx	no special use
SW981	abony (/b, enx serum, in a transduction exp't X— typhimurium, this probably irrelevant)	z_{33}:enx typical phase var.

~~SW-982 and 983, respectively, are 3821-52 and 1553-52 B, (gm).?single factor~~ —x SW 666

~~The latter is, of course, Kauffmann's gallinarum." Not a thing yet from either —x or x— pullorum or gallinarum, either motility or fermentation factors. I don't understand it at all."~~

SW-982	3821--52 —x SW666	B(gm)+	single factors?	
SW-983	1553-52 —x SW666 [Kauff. "gallinarum]	"	"	"

3821-52 and 1553-52 seem very similar indeed. I have not been able to motilize either of them, but FA from them seems to reveal their inherent H type. Until I know more about it, I would not want to say they are **not** gallinarum, whatever that means. I have not yet been able to transduce any marker into or out of any authentic gallinarum or pullorum, so the evidence is still negative. Do you have one or two cultures of maltose-postive variant pullorum?

SW-984 S. javiana —x SW666 B, $1z_{28}$:—
Incidental. I thought this might show less cross-reaction with 1,5 but it doesn't look that way.

SW-985 S. abortus equi #26 —x SW666 a:— (or ?)
It looks as if transduction may serve to unmask otherwise suppressed phases! In previous experiments, typhimurium —x SW666 has given i:-, even though FA came from fairly pure second phase. In experiments —x diphasics, however, only the expressed phase is transmitted through the FA, so there is something special about SW666, perhaps to permit the uninhibited expression of first phases (and never of second). However, the recent :1.2's —x SW666 have not given anything, except occasional b:-, which deosn't tell anything.

SW-988 An alternative phase from #157 that I cannot type. It may go back to 1,2 on repeated transfer in SS agar. Similar attempts to get alt. phases from

#191, etc., have given the same sort of result. Apparently motile bacteria directly from passage in 1,2 serum do not agglutinate in available sera, and revert to 1,2 on further passage in non-serum SS. Could there be an artificial phase with mediocre motility, or is this just the poor development of the H antigen??? I am sending these as SW 891B, 959B and 960B.

S. abortus equi #26 is being looked at as a possible appropriate recipient strain for monophasic second phases, but so far I have been unable to get anything into it at all. If some others turn up, they might do better.

Additional notes:

1) S. bredeney. The following received: 3807-, 4102-, 4641-, 5435-, 5437,6504-, 6612-52, 303-,517-53. Unfortunately, all but 4102-52 are resistant to PLT22, and this is loma-linda, by my own diagnosis, and later as chance would have it that it has the same number as a loma-linda I brought up. Do youhave any others, with or without XXVII? I'm willing to make another stab at it if you are.

2) I am very gratified you had the time to do those absorptions. I can't think of much else that needs doing for our paper (except perhaps verify 1,2:1,5). Would be it be worth verifying the low-titre cross-reactions of some of the transduced phases, as well as excluding poorly reactive residues of the prior, now supplanted phase? E.G., in SW-699, b:1,2 from abony —x typhimurium, does the b phase now react at low titre with z_{35}, 1,6 etc., and is there no trace of i? This may not be a happy example, and in any event I defer to your judgment on the serological necessities.

   Are there any other transductions important to illustrate? As I have asked this repeatedly, I would assume not. May I begin the discuss the mechanics of the paper itself? I can start to map out the contant, which will probably help show up the defects. Meanwhile, could you take responsibility for some sort of introduction? We'll have to pay special attention, together, to the discussion. I don't see how we can avoid discussing the (ir)relevance to problems of nomenclature— think we are in fair agreement about this. I haven't pondered much what journal to prepare this in proper form for [and promise to be better in my syntax in serious writing], but how about J. Immunol? Would this reach the appropriate audience, especially abroad? Meanwhile, what would you think about submitting abstracts to either the Int. Congress (which we probably will not attend, but have a personal repres. in Cavalli) or the SAB in S.F. (won't you be in the vicihity yourself?) We have some few weeks yet to decide about these.

3) Re queries: The only monophasics I have succeeded in using as recipients for FA have been "phase-1" monophasics, including S. typhi, SW666 b:-,and N97 and its 1,2:—. Attempts to substitute second phase factors into these (enx; 1,5; 1,2 other than #157) have failed. One gets either nothing at all, or the latent first phase (vide supra). In one case, b:enx —x 157, 1,2:— gave, instead of enx:— (never seen) or b:— (the usual result), a diphasic 1,2:enx.

4) I am ashamed that I overlooked your ~~Arizona~~ Arizona-Bethesda papers, and have not seen most of them. Could you send me #76,111, 112,113,(123 =111?), 130.139; also 82, as still available. ~~Am~~

Can't think of much else just now. I hope this kind of correspondence doesn't disturb you as much as it would me: I am trying to make it a substitute for more personal discussion. Don't hasten to reply except at your own convenience, wish or opportunity.

Sincerely

Joshua Lederberg

Reqs:

S. bredeney- addnl. strains?

S. pullorum Maltese-positive

Absorptions done with transduced phases ca 3/1/53

Serum	Absorbing strain	Residual Titre for Homologous Antigen
S. dublin	674 phase g,p	<50
S. paratyphi B ph 2	674 phase 1,2	<50
S. dublin	662	<50
S. cholerae suis ph 1	902	<50
S. dublin	667	<50
S. enteritidis	679	<50
IV, V, XII : e, h	668	<50
S. typhi murium	2 under IX, XII : i SW	<50
S paratyphi B, ph 1	670	<50
S. rubislaw ph 1	687	<50
S rubislaw ph 1	683	<50
S. typhi murium ph 1	924 phase i	<50
IV, V, XII : e, h	664	<50
S abortus equi	698 phase e,n,x	<50
S paratyphi B, ph 1	699 phase b	<50
S. abortus equi	925 phase e,n,x	<50
S para typhi B ph 1,2	926 phase 1,2	<50
S abortus equi	926 phase e,n,x	<50

All serums had titers of 5000 to 20,000. In each instance the phases used for absorption were agglutinated to the homologous titers of the serums.

I did not enclose protocols since I thought they would not be included in the paper. They are available if you want them. The important thing seems to be that each phase went into the transducer in unaltered form.

As yet I have not been able to do much about the g,p content of phase 2 of 674. Increased amounts of g,p serum have not affected it. I have not found g,p colonies on replating the phase. I will play with it some more but do not have much hope. It seems to be something like the d,i phase of our paracolons.

I will answer your letter in a day or two but wanted to get these results off to you. Is there anything more you wish me to do?

Can phase 2 be placed in a monophasic host like S. typhi to produce IX, XII : 1,2 or IX, XII : e,n,x? I'm not just clear on this.

Best regards to you & Mrs L.

Phil

The formula for 674 should not be written g,p - (1,2) which would indicate only a part of 1,2 was present. There is no established method of writing a formula to cover such a situation.